EXHIBIT 6

Expert Report of Dr. Charles C. Lanfear

2022-06-02

I was retained by counsel for the defendant to provide rebuttal to expert testimony regarding the Seattle Police Department's (SPD) response to protest activity in June 2020. Specifically, I was asked to evaluate research claiming to provide quantitative evidence for an increase in crime resulting from the decision to evacuate the SPD East Precinct Building on June 8, 2020 until July 1, 2020. This report focuses specifically on Piza & Connealy (2022) and the related expert witness report by the lead author Dr. Eric Piza.

This report is based on materials I have reviewed to date. I reserve the right to revise or amend this report if review of further materials leads me to revise my opinions.

Please note that this report may include citations to and discussion of information and documents that may be considered confidential, subject to a protective order, or otherwise not suitable for public release.

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2. BACKGROUND AND QUALIFICATIONS

The opinions expressed in this report are based on my training, education, and research experience in the field of quantitative criminology. My qualifications are documented in the curriculum vitae attached to this report. I briefly elaborate here on elements of my background that qualify me to evaluate the present evidence and opinions of Dr. Piza.

I am a Research Fellow of Nuffield College in the University of Oxford, and a Postdoctoral Researcher at the Centre for Social Investigation at Nuffield College. In September 2022, I will begin an appointment as a University Assistant Professor at the Institute of Criminology in the University of Cambridge. I received my PhD in Sociology from the University of Washington (UW) with a concentration in Social Statistics from the Center for Statistics and the Social Sciences. I also have a master's degree in public policy from Oregon State University with a focus in criminal justice policy and econometric methods for causal inference.

I am an expert in research design and statistical methodology for estimating causal effects. This expertise is demonstrated by Lanfear et al. (2020), "Broken Windows, Informal Social Control, and Crime: Assessing Causality in Empirical Studies" published in the *Annual Review of Criminology*, the highest impact factor journal in the discipline of criminology and a guidepost for future research. This article uses the potential outcomes causality framework—the dominant approach to evaluating causal claims in the social sciences—to evaluate empirical research on causes of neighborhood crime rates using both experimental and observational research designs. I have also served as a reviewer for articles applying causal inference methods in some of the most rigorous journals in the disciplines of criminology and sociology, including *Criminology*, *American Journal of Sociology*, *Journal of Research in Crime and Delinquency*, and *Justice Quarterly*.

I also have expertise in analyzing data on policing, having been hired as a consultant by the Seattle Police Department and the UW Office of the President to provide independent analyses of racial bias policing. I have also published a number of empirical journal articles using police-recorded crime data, including an earlier time period of the same data used by Piza & Connealy (2022) (e.g., Lanfear et al. (2018)).

3. COMPENSATION

My fee for consultation in this case is $450 per hour, plus reimbursement for any expenses incurred.

4. REVIEWED MATERIALS

- Piza & Connealy (2022)
- Expert witness reports
  - Seth Stoughton, Apr. 28, 2022
  - Eric Piza, Apr. 27, 2022
  - Michael Freeman, Apr. 28, 2022
- Deposition transcript, Thomas Mahaffey, Jan. 26, 2022 (with exhibits)

- Deposition transcript, Carmen Best, Nov. 9, 2021 (with exhibits)
- Deposition transcript, Jenny Durkan, Dec. 8, 2021 (with exhibits)
- "Updates on Capitol Hill from the City of Seattle", Kamaria Hightower, Jul. 1, 2020
- "July 1 Events Incident Action Plan", Seattle Police Department
- "Press Conference, July 1, 2020", Mayor Jenny Durkan
- "City of Seattle Executive Order 2020-08", Mayor Jenny Durkan
- Emails
  - "Chief's Message", Carmen Best, Jul. 1, 2020
  - "Re: Round 2", Lauren Truscott, Jul. 1, 2020
  - "Situation Report – July 28, 2020", Tyrone Davis, Jul. 2, 2020
  - "Re: Cal Anderson, Clean-up, Coordination Meeting", Idris Beauregard, Jul. 1, 2020
  - "C20-983 TSZ Order", Thomas S. Zilly, May 9, 2022
- Seattle Police Department Crime Data: 2008-Present, accessed May 13, 2022 from https://data.seattle.gov/Public-Safety/SPD-Crime-Data-2008-Present/tazs-3rd5

5. OVERVIEW OF OPINION

Piza & Connealy's study, "The effect of the Seattle Police-Free CHOP zone on crime: A microsynthetic control evaluation," is a thorough and thoughtful empirical study using a statistical method appropriate for measuring the effect of events like policy changes on outcomes like crime. Unfortunately, due to the circumstances surrounding the events in and around the East Precinct service area, the causal effect the authors are interested in measuring—the effect of the SPD withdrawal from the East Precinct Building on counts of crime—cannot be estimated using this statistical method. Rather than estimating the effect of SPD's withdrawal from the East Precinct Building on crime, Piza & Connealy instead estimate how much crime increased due to the combination of two factors: a large-scale protest against police and resulting changes in police activity. That is, they are comparing what occurred in the CHOP zone and East Precinct service area to a counterfactual scenario where there was neither a large-scale protest against police nor a withdrawal from the East Precinct Building conducted with the intent to de-escalate protest-related conflict. Critically, their study does not measure the difference between observed levels of crime (i.e., given SPD's withdrawal from the East Precinct Building) and levels of crime that would have been observed had SPD continued to operate out of the East Precinct Building. As a result, their study does not answer the key question: whether levels of crime would have been lower in the CHOP zone and surrounding areas had SPD remained in the East Precinct Building between June 8, 2020 and July 1, 2020.

The first half of this report provides an overview of how causal effects are estimated from observational data. The second half explains why the study by Piza & Connealy fails to meet the criteria that must be used to estimate the causal effect of SPD's withdrawal from the East Precinct Building on counts of crime in the CHOP zone and surrounding areas. Additionally, I document some concerning issues with data validity in Piza & Connealy's study.

6. CAUSAL INFERENCE

Causal inference—the process of estimating the effect of something on an outcome—is challenging. Causal inference is fundamentally focused on the comparison of two quantities, sometimes referred to as "potential outcomes" (Morgan & Winship, 2015; Rubin, 2006): (1) the (factual) outcome we observe given events that actually occurred, and (2) the (counterfactual) outcome we would have observed if a different, well-defined set of events happened to the same units under observation. We call a particular set of events of interest that influence an outcome a "treatment". What is sometimes called the "fundamental problem of causal inference" (Holland, 1986) is that for those who received the treatment, we never observe what their outcome would have been had they not been treated; conversely, for those in the untreated group, we never observe what their outcome would have been had they been treated. Because we do not simultaneously observe both outcomes, we cannot calculate the difference between them for any single unit (the individual causal effect of the treatment). Thus, the main challenge of causal inference is to estimate the unobserved counterfactual outcome—the outcome that *could* have happened but *did not*—so that we can compare it to the actual outcome. The validity of any causal claim thus rests on the proper estimation of a reasonable counterfactual. Put simply, we observe what actually happened, so the problem for causal inference is estimating what we do not observe—the outcome of interest that *did not happen*.

6.1. Estimands

When making causal inferences, researchers are interested in one or more different types of causal effects, often referred to as causal estimands. An estimand is the target quantity that we intend to estimate. This is in contrast to the estimate, which is the value obtained by trying to calculate the estimand using a particular method and data. On the one hand, if we are interested in knowing the average effect of a particular treatment on all units in the population—regardless of whether we actually treated them or not—our causal estimand of interest is the "average treatment effect" or ATE. The ATE reflects the average difference in outcome we would expect any randomly selected unit to experience if we treated it. On the other hand, if we are interested in the effect a treatment produces only on the units we actually treated—and not untreated units—our causal estimand of interest is the "average treatment effect on the treated" or ATT. Less commonly used is the average treatment effect on untreated units (ATUT)—this is the average effect our treatment would have on the units that did not receive any treatment.

6.2. Ignorability

In controlled laboratory experiments, estimating causal effects is often straightforward: We can take two units that are identical on all characteristics (e.g., cloned plants) and treat only one (e.g., inoculate it with a disease), then compare their outcomes (e.g., how long each survives). Again, we never observe the untreated outcome for the treated unit, or the treated outcome for the untreated unit. But, because we believe the units are identical, we can assume the outcome for the untreated unit reflects what would have happened had the treated unit not received the treatment. When we cannot use identical units, such as when our units are people in a medical trial, we can take a large number of units and randomly assign treatment to each one. When treatments are randomly assigned, on average the treated and untreated groups will be equivalent—the unobserved factors influencing their outcomes cancel out on average by being roughly evenly distributed between the groups. This is the theoretical basis for randomized controlled trials (RCT), the gold standard for establishing causal effects in the biological and medical sciences (and when practical, the social sciences).

When treatments are randomly assigned to units, it satisfies a key assumption of causal inference methods: ignorability of treatment assignment, typically shortened to ignorability. Formally, ignorability means the probability of receiving the treatment is unrelated to the strength of the causal treatment effect. Stated another way, the assumption of ignorability is violated if the treatment would have a smaller (or larger) effect on the untreated units than it has on the treated units. This assumption is called "ignorability" because, if satisfied, it means we can *ignore* how the treatment was assigned to units. When we randomly assign treatments, units that would experience larger or smaller causal treatment effects are equally likely to receive treatment, so on average there will be no difference between them—this makes treatment assignment ignorable. To refer back to the causal estimands of the last section, when treatment assignment is ignorable, the average treatment effect (ATE) is the same as the average treatment effect on the treated (ATT) and the average treatment effect on the untreated (ATUT). Because we can "ignore" how treatment was assigned, we can assume there is no difference between treated and untreated units, so their causal effects will be the same. When treatment assignment is ignorable, we can calculate the treatment effect as the average difference in outcomes between treated and untreated units.

When ignorability is violated, we cannot calculate causal effects as the difference between treated and untreated units, because the treated and untreated units actually have different average differences between the outcomes they would experience when treated versus when untreated. Again in terms of causal estimands, this means the average treatment effect in the population is different from the average treatment effect on the treated units (and the ATUT as well). That is, the treated units will have a higher (or lower) effect of treatment than the untreated units.

This is very technical, so an illustration may be useful. Imagine a police department is interested in seeing whether body-worn cameras reduce civilian complaints about police use of force—for example, if cameras result in officers being less likely to use force during an encounter. If the department randomly assigns officers to wear body-worn cameras, they can estimate the effect of cameras as the difference in civilian complaints against officers wearing them and complaints against officers not wearing them. This works because of the random treatment: On average, the treated (i.e., camera-wearing) and untreated (i.e., not camera-wearing) officers will be the same. The average treatment effect (i.e., on all officers) is equal to the average treatment effect on the treated (i.e., those wearing cameras), because randomization makes the treated group effectively equivalent to the untreated. Consider if, instead, the police department offered officers body-worn cameras that they could wear voluntarily and then compared complaints against those wearing cameras to those not wearing cameras. Presumably officers that choose to wear body-worn cameras are different from officers that do not choose to wear body-worn cameras. If the department attempted to estimate the average treatment effect as the difference between officers wearing cameras and officers not wearing cameras, they would receive an invalid result—the treatment is not ignorable.

6.3. Conditional Ignorability

To return to the body-worn camera example, the department could attempt to estimate the average treatment effect of cameras when they are not randomly assigned by adjusting for characteristics of officers that make them more or less likely to wear cameras and more or less likely to receive civilian complaints. As one can imagine, there are many potential factors that might influence decisions to wear cameras and the likelihood of receiving complaints. It would be very challenging to obtain valid measurements of all relevant factors to achieve conditional ignorability in this example.

6.4. The ATT and Time Series

As stated earlier, to make causal inferences one needs to compare observed outcomes with estimated, counterfactual outcomes. But exactly which counterfactual one should estimate is an important question. For example, assuming conditional ignorability, we can estimate the potential outcome of treated units in the counterfactual scenario where they are not treated (this leads to the ATUT). Or, we can estimate the potential outcome of untreated units in the counterfactual scenario where they are treated (this leads to the ATT). When treatment assignment is ignorable, either approach will lead to equivalent results. But, in some cases, this choice is crucial and different approaches yield different results.

This is often the case when the focus is on the impact of some treatment that occurs over a time period on the trajectory of an outcome (i.e., multiple observations of the outcome after treatment). For example, the effect of the SPD withdrawal from the East Precinct on the subsequent trajectory of police-recorded crimes. In this situation, the focus is on within-unit change over time—that is, for any given treated geographic location, what is the impact of the treatment on changes in that same location's future trajectory of crimes? Accordingly, we want to compare the observed changes in the treated units with the counterfactual change those same units would have experienced had they not received the treatment. Because we are only interested in what occurred in the treated units, we do not need to estimate the counterfactual outcomes of the untreated units if they had been treated. In other words, our causal quantity of interest is only the ATT, not the ATE.

How can we estimate the counterfactual change those treated units would have experienced had they not been treated? Rather than comparing our treated units to all units, calculating the ATT requires creating a specific control group to serve as a comparison. If we assume that the trajectories of the treated group and some control group generally follow the same shape, except for differences caused by the treatment, then we can use the trajectory for the control group as an estimate of the counterfactual trajectory in the treatment group—that is, we assume the control group's trajectory is what the treated group would have exhibited if the treatment had not occurred. The difference between the control group and the treatment group provides an estimate of the ATT. The challenge here is that this requires a perfect control group. Only a control group following the same trajectory prior to the treatment group serves as a reasonable estimate of the treatment group's counterfactual. Put another way, this approach involves comparing the treated units to a selection of other units that are very similar to the treated units but that did not receive the treatment. Again, and importantly, using this control group as a counterfactual requires assuming the control group would also exhibit the same post-treatment trajectory in the outcome as the treatment group would were it not for it receiving the treatment. This is often referred to as the parallel trends assumption, because it means assuming that, absent the treatment, the control and treatment would exhibit parallel trajectories of the outcome. This approach is invalidated if it is likely the control and treatment group would have exhibited different (non-parallel) post-treatment trajectories.

7. PIZA & CONNEALY (2022)

The following sections apply the causal inference principles in the prior section to illustrate methodological flaws in the analysis of Piza & Connealy (2022). The most severe problems in the study are documented in the first two sections—ignorability violations and invalid counterfactuals. These problems result in their analysis method being unable to estimate the effect of SPD's

withdrawal from the East Precinct on crime. The remaining sections detail other significant issues that bolster this opinion or highlight additional technical issues that call into question their data and estimation methods.

In the following sections, when I use the term "CHOP zone", I refer specifically to the boundary defined by Piza & Connealy, and not to any other use of the term. Similarly, I use the term "CHOP period" to refer to Piza & Connealy's operationalization of the time period June 8 through July 1. "SPD's withdrawal from the East Precinct Building" refers to the evacuation of the East Precinct Building and modification to certain police services in the areas under examination beginning in the afternoon of June 8, 2020.[1]

7.1. Ignorability violations

Recall that the causal effect of a treatment cannot be accurately estimated unless treatment assignment is (conditionally) ignorable. In the present case, ignorability would be violated if SPD's withdrawal from the East Precinct Building increases crime more (or less) than a similar action would affect crime in the control units. Piza & Connealy use a series of variables to generate a synthetic control group comparable to the CHOP zone, the area within 1304 feet of the CHOP zone, and the East Precinct service area as whole. To refer back to the section on The ATT's and Time Series, they are constructing a synthetic control group to use as an estimate of the counterfactual for the treated group. Specifically, they use the following measures of each street segment:

- Whether the street is a principal or arterial roadway
- Whether the pre-CHOP period total crime was over the 80th percentile of all street segments in Seattle
- Whether the police beat the street segment is in was over the 80th percentile of police calls
- The total count of commercial businesses
- Total consumer facing establishments
- High-crime location quotient, a measure of crime density in the blockgroup
- The total length of the street segment
- Whether the street segment is in a high-disadvantage (e.g., poverty) census blockgroup
- Whether the street segment is in a census blockgroup with a high sum of the percentage non-white residents, residents age 15-29, vacant homes, and owned/rented homes

The microsynthetic control method assumes that, if not for SPD's withdrawal from the East Precinct Building, street segments in Capitol Hill would have on average the same trajectory of crime as street segments elsewhere in Seattle that are equivalent on these measured characteristics. Put another way, this method of analysis assumes all relevant differences between Capitol Hill and other parts of the city can be accounted for with these measures and the prior crime count. As Piza states in his expert report (p. 2):

> A key consideration in evaluation research is the nature of the comparison area used to assess changes in the intervention area. Analysis findings are considered to be statistically significant when (and only when) crime trend changes in the intervention

[1] Changes to police responses are documented in detail in the deposition of Thomas Mahaffey and related exhibits 2-5.

> area substantially differ from the comparison area. … However, the comparison area must be similar to the intervention area across variables than [sic] may influence crime changes (e.g. prior crime rate, land usage, economic wellbeing, etc.) for results to be valid. The more similar intervention and comparison areas are to one another, the more researchers are making 'apples to apples' comparisons.

When SPD withdrew from the East Precinct Building, there was an ongoing large-scale protest against the police focused on the East Precinct Building. The withdrawal was, in fact, a response to increasingly intense protest activity. Additionally, this protest resulted in a large increase in the number of people present—one of the strongest predictors of crime (Cohen & Felson, 1979). As Plaintiffs' expert witness Michael Freeman notes on page 9 of his report, "The police evacuation from the CHOP zone occurred during a period of time with a rapidly increasing population density. Protesters and counter-protesters accumulated leading up to and during CHOP, leading to an unprecedented population density in the area." There was not a similar large-scale protest against police occurring during the CHOP period in any of the units which were synthetically combined together to produce a control group. There is no reason to expect that, absent SPD's withdrawal from the East Precinct Building, the CHOP zone and surrounding areas would have experienced typical levels of crime—that is, it is unlikely it would have followed a typical trajectory whether or not SPD withdrew from the East Precinct Building. In fact, during the week prior to SPD's withdrawal from the East Precinct Building, police-recorded crime in the CHOP zone and the area served by the East Precinct rose substantially, while crime did not rise similarly elsewhere in the city.

It is thus unlikely, in my professional opinion, that the effect of a similar withdrawal from these synthetic control units would have been similar to the effect of SPD's withdrawal from the East Precinct Building during the large-scale protest against police. If this is the case, then Piza & Connealy's microsynthetic control method does not estimate the counterfactual outcome that would have occurred had SPD not withdrawn from the East Precinct Building, and thus the method cannot estimate the causal effect, if any, of SPD's withdrawal from the East Precinct Building. Rather, Piza & Connealy are at best estimating the simultaneous effect of the protest and all police responses to the protest—but only for June 8, 2020 onward.[2] Put another way, using the technical language of the prior sections, Piza & Connealy cannot calculate the average treatment effect on the treated units—the causal estimand they are interested in—because the control units are not in fact similar to treated units in all relevant ways except for the treatment. It is instead likely the control and treatment units would exhibit very different trajectories of crime if the SPD withdrawal from the East Precinct Building had not occurred. This results in a violation of the parallel trends assumption that their microsynthetic control method requires to produce valid estimates (i.e., they cannot estimate the ATT they are interested in).

Further, the fact that the protest became concentrated in Capitol Hill, specifically near the East Precinct Building—and not elsewhere in the city—suggests the area has a different relationship with regard to police than other areas which are being weighted together as synthetic controls. If a protest that forms in Capitol Hill—or near the East Precinct Building specifically—is more likely to result in conflict with the police, public disturbances, and other criminal offenses, then any treatment that

[2] If the authors were interested in the causal effect of the entire protest and police response, they would need to extend their treatment period to at least one week earlier. I conducted this reanalysis and note my findings in the Pre-Intervention Trends section below.

occurs as a *result* of the protest cannot be treated as if (conditionally) randomly assigned (i.e., treatment is not ignorable). It is worth noting there was a large spike in police-recorded crime in the CHOP zone, the area around the CHOP zone, and the area served by the East Precinct on July 25, 2020 (see figure **1** in the Pre-Intervention Trends section). This is another protest event, this time regarding the King County Juvenile detention center. Similar to the last day in the CHOP period (July 1, 2020), this spike occurs only in areas served by the East Precinct—but importantly occurred in the absence of a police withdrawal. The simple fact that large protests that generate conflicts with police—and resulting increases in police-reported crime—occur in the vicinity of the area where the CHOP appeared suggests it is not comparable to a weighted combination of other locations in the city. If this is the case, Piza & Connealy are using an invalid counterfactual as a comparison, which invalidates their statistical estimates. The next section considers the validity of their counterfactuals more deeply.

7.2. Invalid Counterfactuals

Piza & Connealy's stated goal is to estimate the effect of SPD's withdrawal from the East Precinct Building on levels of police-recorded crime in street segments in the CHOP zone, the area within 1304 feet of the CHOP zone, and the East Precinct service area as a whole. These are all ATTs. The observed outcomes here are the number of police-recorded crimes occurring in the CHOP, area around the CHOP, or East Precinct service area as a whole, during the period June 8, 2020 through July 1, 2020. The counterfactual they are interested in is the number of police-recorded crimes that would have occurred in these areas during the same period absent the SPD withdrawal from the East Precinct Building. The validity of their analysis rests on their ability to accurately estimate the counterfactual outcomes: The weekly counts of police-recorded crimes that would have occurred in each zone if SPD had not withdrawn from the East Precinct Building.

A basic question is: what do Piza & Connealy envision as the counterfactual state had SPD not withdrawn from the East Precinct Building? Piza & Connealy do not define this counterfactual state in the paper. They provide only what they believe police should have done instead (p. 52): "A more prudent solution would have been to more effectively engage with protesters to quell the violent activity." But, as the report by expert Stoughton notes on page 11, SPD made repeated efforts to engage with protesters and de-escalate the situation. These efforts were unsuccessful. Piza & Connealy also suggest police should not have used crowd control weapons in the days prior to the pullout because they "may have contributed to the uprising", but that their existing tactics (including, presumably, using crowd control weapons) may have been effective if barriers had remained in place. Different sets of responses would generate different counterfactual outcomes: If the police remaining in the precinct resulted in escalation, police-recorded crime would undoubtedly be higher. Escalation could also easily produce different and more severe incidents, such as attempted arson—as documented by expert Stoughton (pages 10, 15, 35)—and physical injuries or deaths from clashes with police.

A related question is, what counterfactual state are Piza & Connealy actually estimating with their synthetic control? SPD's withdrawal from the East Precinct Building was a response to disorder and hostility toward police as part of a large-scale protest against police. As Piza & Connealy state on page 1, "SPD abandoned the precinct in an attempt to quell the confrontations, resulting property damage, and injuries to both police officers and protestors." The counterfactual synthetic control units are constructed by matching street segments in the areas of interest (i.e., CHOP zone, the area around the CHOP zone, and the East Precinct service area) to segments outside those areas on

characteristics in the year preceding the police pullout. As explained in the discussion on conditional ignorability, it is notable that in virtually the entire preceding year, street segments elsewhere in the City of Seattle were not subject to large-scale protests against police. Effectively, then, Piza & Connealy are comparing areas subject to a large-scale protest against police to areas not subject to the same protest, but attributing all observed differences between the areas to SPD's withdrawal from the East Precinct Building which occurred as a result of the large-scale protest against police.

In short, the effects of the protest itself and the policing removal are not separable with the methods in use, because the control units did not experience similar extended and concentrated protests in the lead-up to the treatment. As a result, Piza & Connealy's model is actually estimating the difference between the observed counts of crime in the CHOP zones and a counterfactual where neither the protest nor SPD's resulting withdrawal from the East Precinct Building occurred. It should be readily apparent that this is not the comparison that Piza & Connealy say they are interested in estimating—they are using the wrong counterfactual. As the validity of all causal inference methods rests on the estimation of the correct counterfactual, this is fatal to their analysis.

7.3. Pre-Intervention Trends

To reiterate an earlier point, a basic assumption of the microsynthetic control method is that units that have similar observed characteristics, including pre-treatment patterns in their outcomes, are also similar on all relevant unobserved characteristics that matter for producing outcomes in the future. That is, these units that are similar on observed characteristics can then be expected to exhibit similar patterns in the future unless affected by some external shock—in this case SPD's withdrawal from the East Precinct Building. A complication for the application of the microsynthetic control method is that trends in crime across the city became very irregular immediately prior to the CHOP period. As I have noted repeatedly, the area around the East Precinct Building was subject to a large scale protest against police in the period immediately prior to SPD's withdrawal from the East Precinct Building. Police-recorded crime increased sharply in the Chop zone in the week before the SPD's withdrawal from the East Precinct Building (and in many other US cities around this time period). But police-recorded crime also decreased sharply immediately before this period across most of the rest of the city. This is likely a reaction—much like the protests—to the murder of George Floyd on May 25, 2020. The reaction to the murder of George Floyd may represent a large-scale change in patterns of crime and police reporting—what criminologists might call a structural break—that invalidates comparisons made between the pre- and post- period.

We see in Figure **1** that police-recorded crimes change enormously citywide after May 25.[3] Police-recorded crimes first spike then drop to a lower level than in any other period in over a year (though this early spike does not occur in the CHOP zone). This is likely due to a mix of inseparable factors such as reduced reporting due to lack of confidence in police, diversion of police attention to protests, etc. In the synthetic control method, weighted values of these unusually low counts are being used as a comparison. Much of the estimated treatment effect may be stemming not from increases in crime in the CHOP but rather decreases elsewhere in the city.

[3] I omit fraud offenses in this plot. See the section on fraud offenses for an explanation of why these offenses should be omitted.

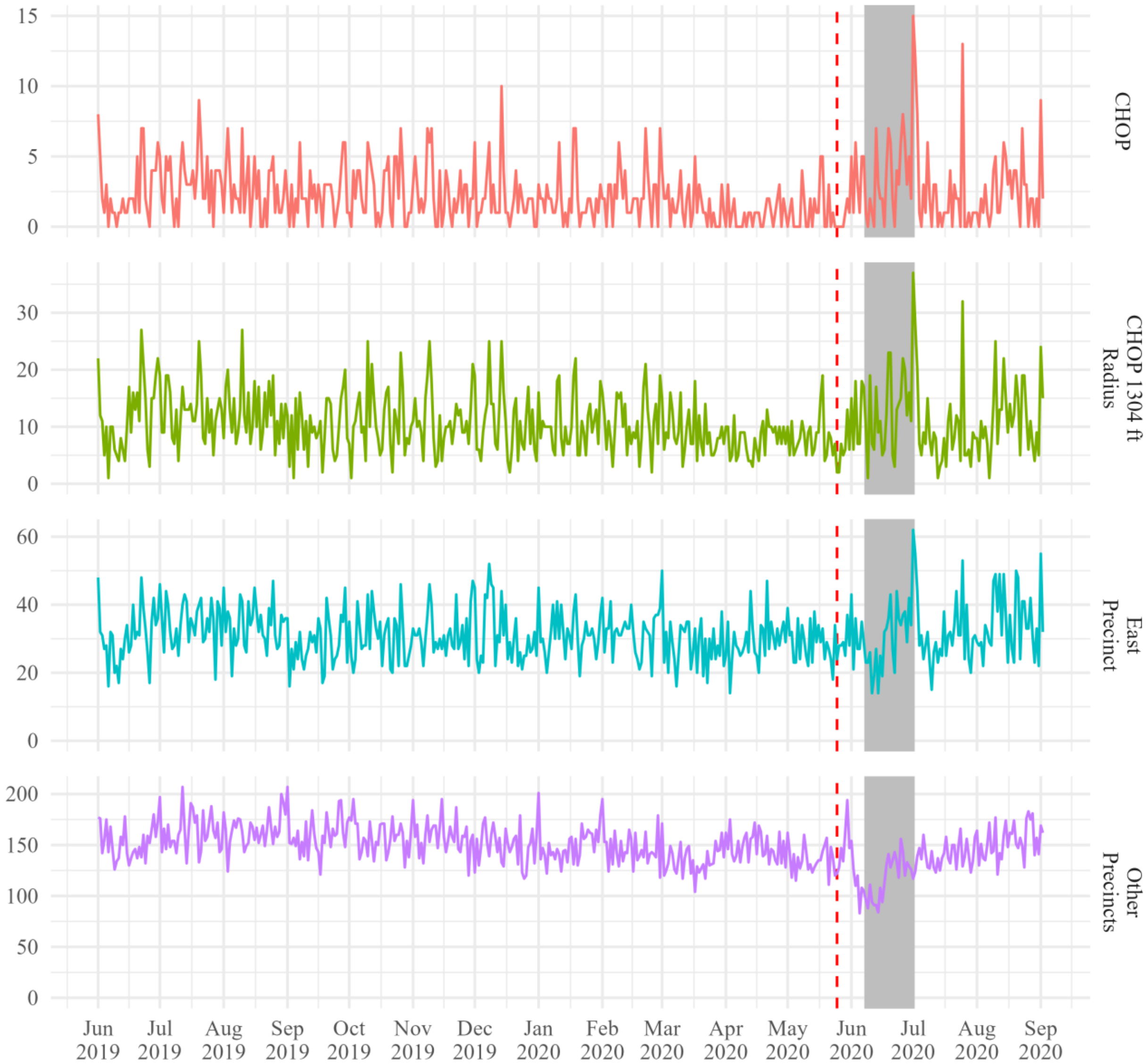

FIGURE 1 *Count of SPD-recorded offenses by zones defined by Piza & Connealy (2022). Dashed red line is the date of the murder of George Floyd. Gray box is the CHOP period. Fraud offenses omitted*

Replicating Piza & Connealy's microsynthetic control analysis but beginning the treatment period in the six days before the SPD withdrawal from the East Precinct Building produces a statistically significant treatment effect for the CHOP zone in the week prior to the withdrawal (174.1%). That is, the count of crimes in the area where the CHOP would emerge was already much higher than in comparable areas elsewhere in the city. This means that, by Piza & Connealy's own method, crime in the CHOP zone was much higher than expected before the SPD withdrawal from the East Precinct Building. Further, according to this reanalysis, police-reported crime in the CHOP zone was actually lower in the first week following the withdrawal from the East Precinct than in the synthetic control units (9.4% lower). By the end of the CHOP period, according to Piza & Connealy's method, the CHOP zone had only just returned to the level of police-reported crime seen prior to SPD's withdrawal from the East Precinct Building. The increase in the week before SPD's withdrawal from

the East Precinct Building was larger, in percentage terms, than the crime increase found in any week in the CHOP period.

Regardless, the key question is not if there was more crime in the CHOP zone during the CHOP period than in prior periods, but rather if crime would have been lower without SPD's withdrawal from the East Precinct Building but while the same conditions otherwise occurred—which includes a large-scale protest against police featuring larger than usual transient population including many individuals hostile to law enforcement. That crime in the area was going up prior to the police pullout suggests this is not the case. It is further complicated by evidence that police-recorded crimes increased greatly the day police returned (in force) to the East Precinct Building and began clearing the CHOP zone. The following section discusses this problem.

7.4. Inclusion of July 1, 2020

Piza & Connealy's decision to include the entire day of July 1, 2020 is questionable. SPD deployed to the area around the East Precinct Building in force on the morning of July 1, and began clearing the CHOP at approximately 5:00 AM. According to "Situation Report – July 28, 2020" by Lt. Davis, as a result of redeployment to the CHOP and East Precinct, SPD conducted 43 arrests between 3:30 AM and 4:30 PM, and 25 arrests between 4:30 PM and 6:00 AM the following morning. This means not only was July 1 almost entirely a policed day, it was a day in which there was a high degree of concentrated police attention.

It is not a coincidence that July 1, 2020 is the day with the highest count of police-recorded crimes in the treatment zones in Piza & Connealy's data. The plot below depicts daily counts of crime for the CHOP zone from Piza & Connealy's day 1 (June 1, 2019) to day 396[4] (July 1, 2020) data. We see here that July 1, 2020 (the last day in the series) is an obvious anomaly, even within the CHOP period.

[4] Technically this should be day 397, but, as noted in the section Misreported Analysis Timeline, Piza & Connealy apparently omit February 29, 2020 by mistake.

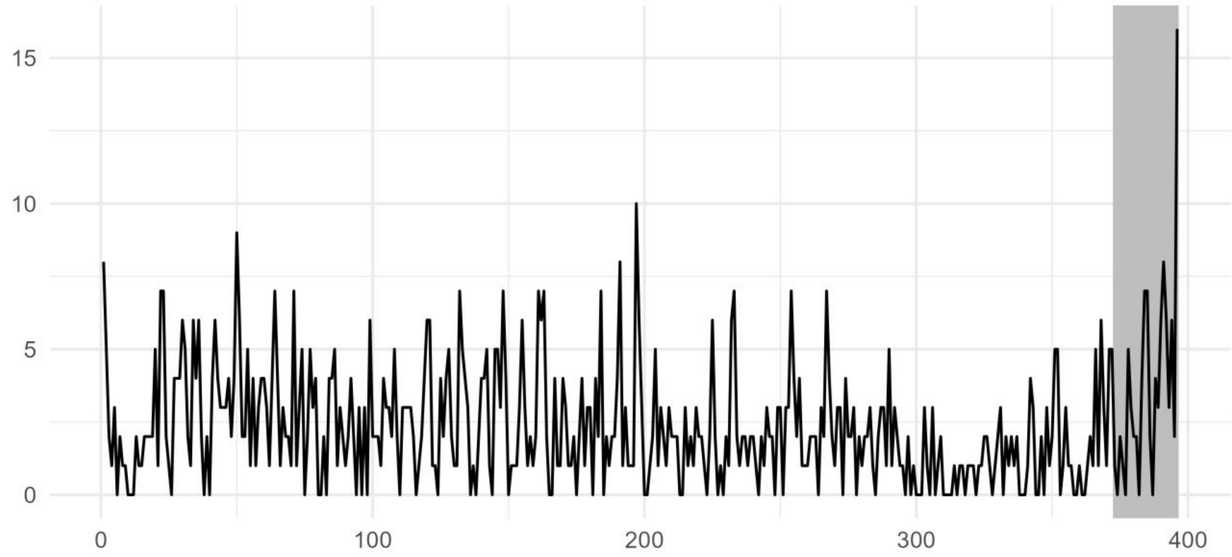

FIGURE 2 *Count of offenses per day within CHOP zone. Gray box is the CHOP period*

If you remove this day from the analysis, as in Figure **3** below, you can see the CHOP period is similar to periods prior to COVID-19 (the low points roughly starting around day 300). Similarly, if we rerun Piza & Connealy's model for the CHOP zone but replace the value for July 1 with values for an average CHOP-period day (3 crimes), the estimated causal effect is reduced by a third, from 132.9% to 90.4%.[5]

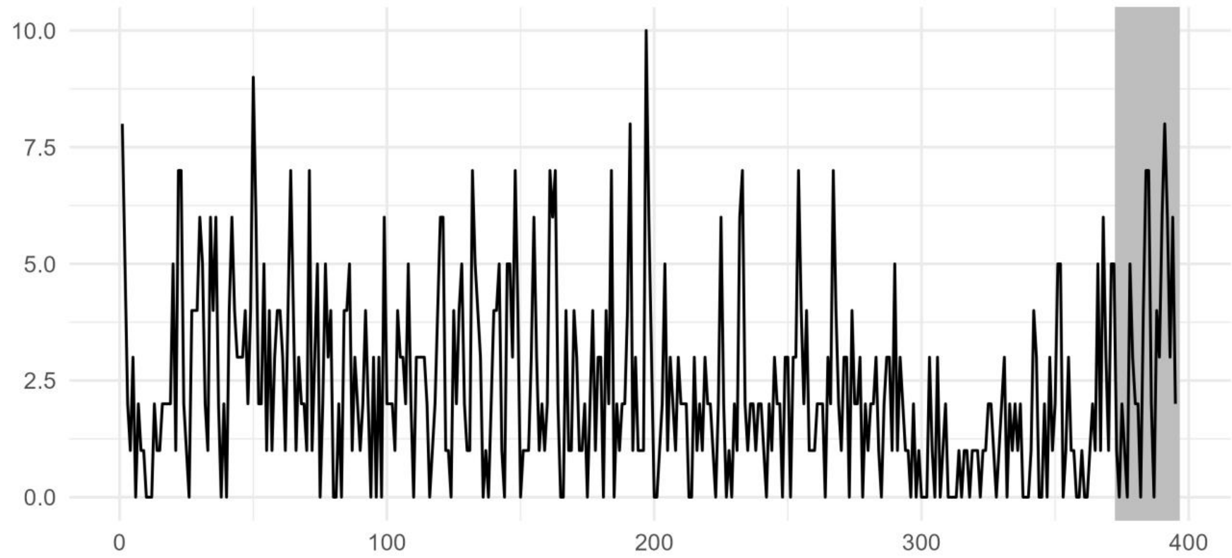

FIGURE 3 *Count of offenses per day within CHOP zone excluding July 1, 2020. Gray box is the CHOP period*

[5] The day cannot be removed entirely—as it should be—because their method is restricted to 6-day weeks.

Figure **4** displays Piza & Connealy's data aggregated to 6-day weeks as done in their analyses. Week 60, the first displayed, begins May 21, 2020 and includes the date of the murder of George Floyd. Major protests in Seattle began the following week and major conflict near the CHOP zone occurred mainly in week 62. Note that police-recorded crimes in the CHOP zone declined sharply in the week beginning with SPD's withdrawal from the East Precinct Building (week 63). Crime counts in the CHOP period did not exceed those in the preceding week (when protestors engaged with police) until the last week the CHOP was active—but 16 of the 41 crimes (39%) recorded that week occurred on July 1, the day SPD returned to the East Precinct Building. If we look to the publicly-accessible SPD offense data, we see that of the 16 police-recorded crimes on July 1, 2020, only two occurred prior to 5 AM, when SPD was deployed to the area. Further, 13 of those 14 recorded offenses after 5 AM appear likely to be the result of conflicts between police and remaining protesters that did not disperse: 7 assault offenses, 4 vagrancy violations, and 2 weapon law violations. The remaining police-recorded crime is one theft offense. Again, this suggests July 1, 2020 should not be included in Piza & Connealy's analysis, and its inclusion greatly inflates the increase in crime they estimate occurred during the CHOP period—an estimate, that, as noted earlier, does not separate out the effect of the SPD withdrawal from the East Precinct Building from protest activity in the first place.

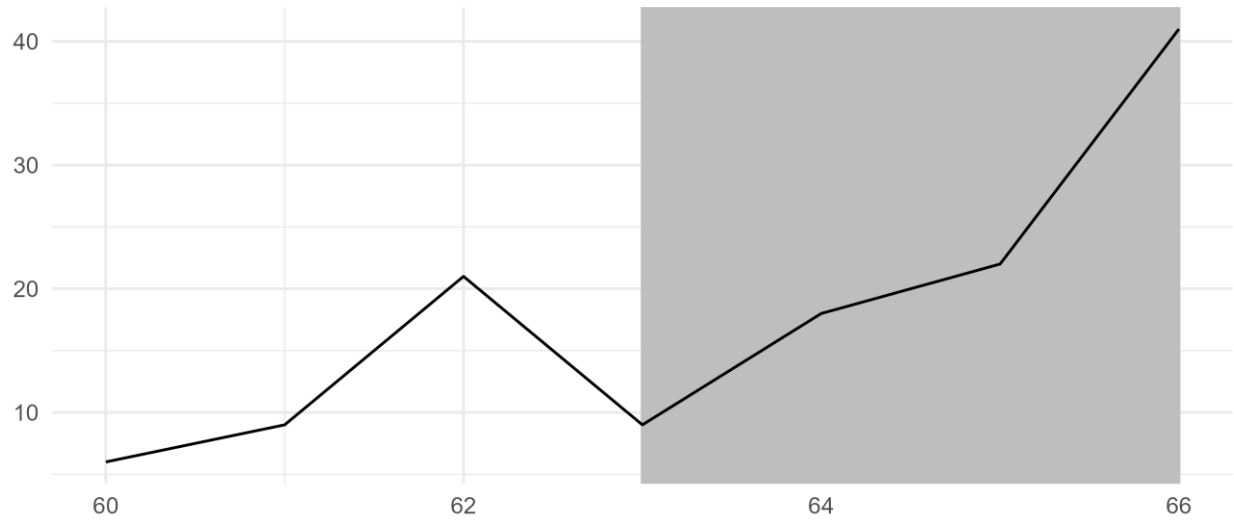

FIGURE 4 *Count of offenses per 6-day 'week' within CHOP zone. Gray box is the CHOP period*

7.5. Fraud Offenses

Piza and Connealy's crime data exhibit highly unusual spikes in daily police-recorded counts of crime as seen in Figure **5**. By considering their data alongside the publicly available SPD data, I was able to determine that the enormous spikes in March and May 2020 are due to single days where very large numbers of fraud offenses were reported (as many as 593 on May 18, 2020). These are presumably cases related to COVID-19 unemployment fraud. These should not be included in their overall counts—they do not represent criminal offenses comparable to those occurring in the treatment zones under examination.

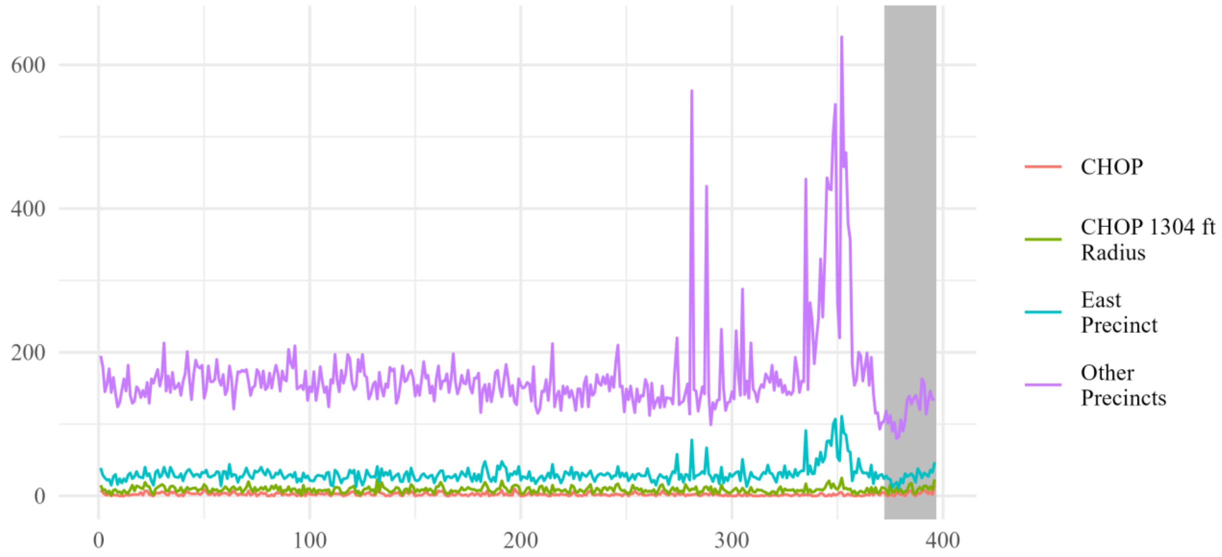

FIGURE 5 *Piza & Connealy (2022) crime counts by day. CHOP period in gray.*

Removing the fraud offenses produces the more stable time series seen in figure **6**.

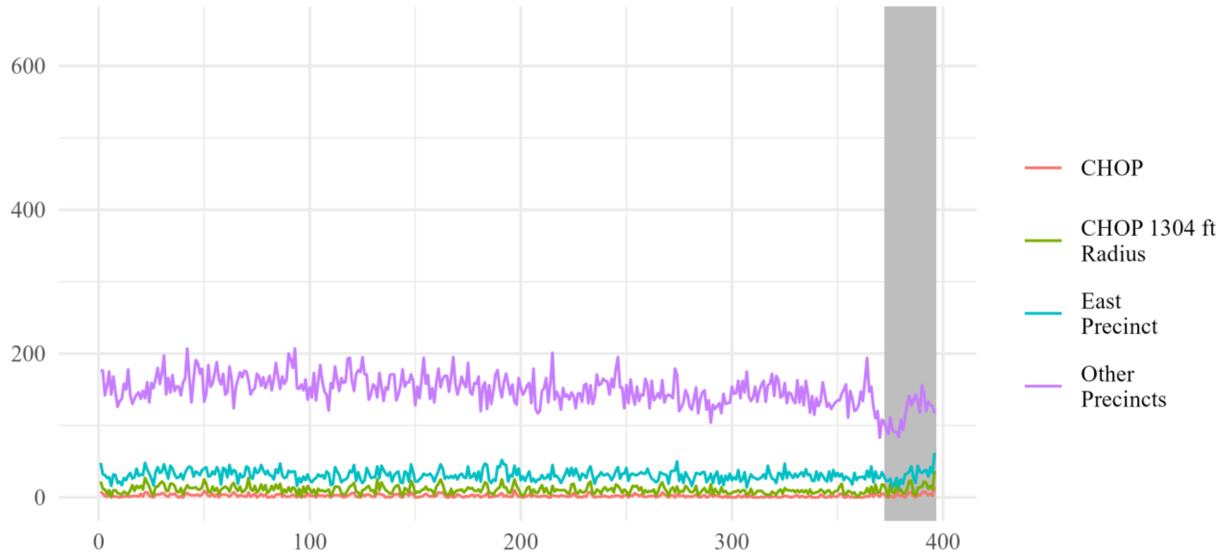

FIGURE 6 *SPD daily crime counts with fraud offenses excluded. CHOP period in gray.*

Unfortunately, I cannot assess the effects of removing fraud offenses on the microsynthetic control estimates because Piza & Connealy's publicly-available replication data do not separate offense types. I cannot construct matching microsynthetic control data using data from SPD with fraud cases removed, because Piza & Connealy do not provide identifying information on their street segment units necessary to link them to SPD data.

7.6. Misreported Analysis Timeline

Piza & Connealy state that "6-day crime count totals were matched on for the identified target zone from 6/1/2019 to 6/7/2020 (62 separate 6-day matching blocks)." 62 6-day blocks is 372 days.

However, if pre-treatment starts on June 1, 2019 and ends on (but includes) June 7, 2020, the pre-treatment period should be 373 days long. I matched total crime counts in their data to SPD records and confirmed their first pre-treatment date is indeed June 1, 2019 and their last pre-treatment day is indeed June 7, 2020. To make this possible, Piza & Connealy would need to have omitted a day from their data by mistake. I suspected this occurred because Piza & Connealy omitted February 29, 2020—a leap day. I confirmed this by checking their daily CHOP crime counts against SPD data: Their day 273 crime counts match SPD data on February 28, 2020 but their day 274 crime counts match SPD data on March 1, 2020. There is no match for February 29, 2020. It appears to have been omitted by mistake. This is unlikely to compromise their analysis because the intended treatment date (June 8, 2020; day 373 in their data) is used correctly, but, like the inclusion of the fraud cases shown in the previous section, it is a troubling error which raises concerns about overall data quality.[6]

8. SUBMISSION

The preceding constitutes my opinions and analysis regarding the opinions set forth in Dr. Eric Piza's April 27, 2022 report and the referenced Piza & Connealy study published in 2022 titled "The effect of the Seattle Police-Free CHOP zone on crime: A microsynthetic control evaluation." My report is based on materials reviewed to date, and I reserve the right to revise this report if subsequent information causes me to change the opinions written above.

Please note that this report may include discussion of materials that are confidential, subject to protective orders, or otherwise not suitable for public release.

Charles Lanfear — Jun 2, 2022

Dr. Charles C. Lanfear — Date

9. REFERENCES

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[6] I also checked the timeline reported in the paper against their publicly available replication code, but the replication code has a different set of incorrect listed dates—the documentation does not match what was actually done. For instance, lines 66 and 104 indicate the analysis period (both pre- and post-treatment) runs from June 1 2019 to July 8, 2020, but the data and analysis functions use June 1, 2019 to July 1, 2020 (dropping Feb 29, 2020).

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